Extortion with Protection: Understanding the effect of rebel taxation on civilian welfare in Burundi

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Abstract

Using a panel dataset from Burundi where information on protection payments during the 10 year civil war were collected, we test the relationship between payments, the nature of extraction by the rebels, and welfare outcomes. We ask, does payment to rebels insure against future welfare loss and does the nature of payment matter? Specifically, does the level of institutionalisation of extraction within the rebel governance structure provide a form of insurance for future welfare? No less than 30% of the interviewees made at least one payment. Rebels extract these taxes through one of two routes: an 'institutionalised' and regular cash-with-receipt method or an *ad hoc* and unpredictable labour extraction. Using matching methods we find that payment through the institutionalised route increases household welfare between 16 and 25%. *Ad hoc* extraction has no effect. We situate our findings in the empirical literatures on contributions to mafia-type organisations and rebel governance.

Keywords: extortion, taxation, forced labour, welfare, rebellion, Africa

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Introduction

In the context of war, weak states and civil unrest, payments to powerful groups - government forces, rebels, militia and mafia – can be extorted or given over voluntarily in exchange for protection, or insurance, against a range of negative outcomes, including death. Furthermore, the nature of extraction of payments reflects structures and aspirations of territorial control of rebel governance, which can be *ad hoc* or institutionalised and sometimes accepted by civilians (Arjona, 2008; Olson; 2000). Whether effective protection is provided is a matter of debate and a review of the literature on the subject suggests that payments can help for protection but are not a guarantee against negative livelihood outcomes.

Using a novel panel dataset from Burundi where data on protection payments during the 12 year civil war (1993-2005) were collected, we test the relationship between payments, the nature of extraction by the rebels, and welfare outcomes. In particular we have two points of interest. First, does payment to rebels insure against future welfare loss? Second, does the nature of payment matter? That is, we are interested to see if the level of institutionalisation of extraction within the rebel governance structure (proxied by predictability as opposed to unpredictability in extortion) provides a form of insurance for future welfare. While payment to rebels does not equate with the usual insurance market conditions, for an insurance market to function smoothly payments and risks need to be predictable in advance. In the same way, if rebels institutionalise a particular form of extraction, this is likely to have better welfare outcomes for the civilians than if the extraction is *ad hoc*. We are interested to test whether this relationship holds under conflict conditions. Olson's (2000) work speaks to the other side of this coin, that is, does 'mobility' of rebel governance

structure (or rebel aspirations for taxation of population) determine the welfare outcomes for economies/societies at large. While intricately related, we are interested in the household level welfare impacts. We provide a review of the rebel governance structure in Burundi as a way of contextualising these outcomes. This is supplemented by qualitative evidence collected by the authors from key informants who had first-hand knowledge of the payment procedures during the civil war period. To the best of our knowledge this is the first paper to investigate, empirically, the linkages between the nature of rebel governance as reflected in type of extortion and household welfare outcomes during civil war.

We use standard OLS models to investigate the relationship between extortion type and welfare. As a way to control for variation in the observable features of the households in our sample, we specify a model that predicts the determinants of extortion and then use this as part of an econometric matching design in order to establish the robustness of earlier findings. We find a strong and robust relationship between extortion and welfare, however this relationship holds only for payments made in cash to rebels and not as extraction for forced labour. The reasons for this are likely to relate to several factors to do with (or absence of) rebel governance: (i) the fact that cash payments were often given in advance, before an act of violence, such as rebel attack, occurred; (ii) such payments were often made regularly, in a predictable way whereby rebels would provide a receipt of payment; (iii) labour was typically extracted in an ad hoc, unpredictable way, and usually at gunpoint, and; (iv) the socio-economic characteristics of persons having cash extorted differs from those having labour extorted. In particular, persons owning an enterprise are more likely to

make cash contributions to rebel groups. This means that rebels know whom to target, or in alternative wording, persons with this profile know that they have to contribute.

In this way the institutionalised forms of tax extraction by rebels, in our case in the specific form of regular cash payment, can be hypothesized to act as an insurance mechanism against negative outcomes for persons with certain economic profiles. Conversely, extortion in the form of irregular extraction (labour in this case) does not provide insurance for future welfare. These findings indicate that the nature of extortion and rebel governance within a conflict scenario is a critical determinant of welfare.

Our results resonate with Arjona's (2008) theory of local orders within the context of civil war, highlighting that the greatest challenges and opportunities for peace, reconciliation and reconstruction vary from place to place. Echoing the work of Olson, our works illustrates the co-dependence of the civilian population and the rebel movement, but fundamentally it shows how institutionalized forms of rebel governance (approximating Olson's 'stationary' bandits) have better outcomes for victims of extortion in terms of security and welfare than simple ad hoc punitive strategies (approximating Olson's 'roving' bandits).

Protection and Extortion: literature review

The literature concerned with protection payments is largely limited to the Italian (Sicilian) and Italian-American Mafias, Russian *Mafiya*, and the Japanese *Yakuza*. There is also a small sub-literature that discusses protection payments in developing countries. Noticeably, the effect that protection payments have on the welfare of the

victims has received almost no attention. Instead authors have concentrated on the history of protection payment organizations, the conditions that create a supply and demand for protection payments, and the operations of protection payment organizations. For purposes of this paper we review what the literature has to offer on (1) the nature of payment/extortion in terms of whether it is predictable or not and, relatedly, rebel governance, and; (2) the effects of payment and the nature of payment on welfare outcomes. Before this a brief note of the distinction between extortion and protection payments is worth making.

A loose definition of how United States Federal Law and Russian legal authorities define extortion is 'the seizure of property with the knowledge and consent of the owner through the use of violence or the threat of violence' (Lotspeich 1997, 22). Theoretically protection payments differ from pure extortion in two ways. First, the definition for extortion omits the provision of protection to the owner. Second, although not clearly defined above, there are cases in which payment is voluntary, for instance in documented cases in West Darfur and Iraq (see Jaspars, et al. 2007, 14; Williams 2009a, 160-161).

While the terms protection payment and extortion are often used to refer to the voluntary and involuntary nature of payment extraction, respectively, studies show that the two terms are often confused and ambiguous (see Gambetta 1993 for a case study of the Sicilian Mafia). The confusion between these activities means that distinction for purpose of analysis is difficult. For instance, Gambetta has shown that extortionists may be forced to provide protection or, as in numerous other cases, the

group receiving the payment may not be able to guarantee their customer's safety. Here, we use the terms interchangeably.

*The nature of extortion and rebel governance*While context-specificity is critical, research shows that where institutions that exist to protect citizens (judicial system, police, military, etc.) are weak, there is an increased need for protection. In the context of war, protection often comes at a cost. The amount and the regularity with which the money or goods is extracted depends on the group extracting and the victim from whom they are withdrawing payment. Unlike many famous economists before him who believed that voluntary, mutually advantageous exchange ruled market relations, Olson (2000) recognises the role that power, coercion and force have in exchange relationships. In his famous book *Power and Prosperity: Outgrowing Communist and Capitalist Dictatorships* Olson considers the role of power and the relation between governance and economic performance. Governance by a "stationary bandit" is more likely to support productive economic activity than governance by "roving bandits" because the former has an "encompassing interest" in maximizing output so that he can maximize his take from that output.

For instance, some evidence suggests that 'stationary' bandits are able to institute protection systems thereby cementing long-term regular revenue. In the case of migrant Chinese businessmen in New York City, the gang member demanding payment and the owner of a business negotiate the amount of money paid in order to prevent damage by the demanding gang or rival gangs. After the amount is negotiated, the business owner will pay regularly, whether it be on certain holidays, weekly, or monthly (Kelly, et al. 1993, 259 and 261). In South Africa, members of the *Mapogo a Mathamaga* pay annually and their payment is based upon their status

and the size of the business. (von Schnitzler, et al. 2001, 14). In Waro and Urdi in West Darfur, villagers paid the Janjaweed 5SDG (1.5US$) per month for protection, while an Iraqi said that he paid US$13 per month to the local Mahdi in order to avoid violence or kidnapping (Jaspars and O'Callaghan 2008, 11; Williams 2009a, 236). In Ottoman Gaza, protection payments to Bedouin tribes were legalized. The villagers benefited from the legal and regular payments, as the rules that were erected prevented contact with villagers, which could have resulted in the demand for further payments (Etkes 2007, 10).

There are a few cases, where victims are forced to pay what can be characterized as "user fees." The military officers involved in coca trafficking require a US$5,000 protection payment for each planeload of Peruvian coca leaves or paste (Mason and Campany 1995, 162). In Somalia, West Darfur, and Iraq, people pay for safe passage on public roads (Vinci 2006, 9; Jaspars and O'Callaghan 2008, 14 and 11; Williams 2009a, 91; Williams 2009b, 332). In West Darfur, pastoralists pay taxes as well as arbitrary payments of up to US$150 in order access markets, while a person may be charged US$900 to take livestock to market with the protection provided by a military escort. Villagers in Abata were forced to pay for the use of camels if they wished to travel to Zalingei (Jaspars and O'Callaghan 2008, 14 and 11).

In some instances, businesses, rather than individuals or households, receive protection. In the Democratic Republic of Congo, RCD-ML rebels "sold" insurances, technical notes and discharge papers to businesses. In addition, by paying a fee to the rebels, businesses reduced or eliminated their taxes (Raeymaekers, forthcoming, 9-10). A farm owner and other members of a cooperative in Beni-Lubero in the

Democratic Republic of Congo pay the Mayi-Mayi one cow per month to pay for their protection (Raeymaekers, forthcoming, 10). There are also cases where villagers pay through a combination of food and money in return for protection (Jaspars and O'Callaghan).

All of the examples above illustrate the various ways in which mafia or rebels establish and maintain governance structures that increase the probability of consistent and sustained extraction as well as citizen/civilian compliance. Sanin and Baron (2005) raises some pertinent issues in his analysis of the evolution of the Colombian, Puerto Boyaca's paramilitary regime. Of relevance to this paper is the question of how violence and repression constitute social order. Indeed, the Colombian paramilitary appeared as a punitive force, basically of the cattle ranchers and the narcotraffickers, but they soon discovered the need to govern, which entailed establishing new mechanisms to control the population. Similarly, Olson (1993) asks how criminality and the private provision of security are related? To Olson (1993) criminality was centrally important to rent extraction and the formation of the modern state whereby an elementary present value calculation shows that it is in the best interest of the bandit to limit his rent extraction, so as to give economic agents the opportunity to accumulate. Gradually, *ad hoc* extortions and rackets become taxes, the need to inspire fear is replaced by the need to control, and big organisational apparatuses appear. Stationary bandits were able to establish a monopoly on rackets and extortion, and at the same time offer them the opportunity of legitimising through the provision of security.

Furthermore, Ana Arjona (2008) argues that in most contemporary civil wars the fight is about gaining territorial control rather than defeating a rival army in successive battles. This affects the ways in which armed groups relate to civilian populations. "When the survival and success of armed groups depend on territorial control, civilian collaboration becomes crucial. Civilians can provide the armed groups with a wide range of valuable resources and endowments, including information, food, shelter, and labour force. Without these resources, armed groups can hardly survive, let alone maintain territorial control. Because civilian collaboration is so essential for armed groups, they have a clear incentive to behave in ways that render it. But collaboration is a complex matter. It may involve only a few occasional actions, or a long list of daily activities; and these behaviours can entail either mere obedience or endorsement. Given this heterogeneity, the effectiveness of violence is limited. If violence cannot bring about the different instances of collaboration that armed groups need from civilians, what is the alternative? Creating a new social order offers great advantages. By creating a new social order the group is able to influence civilians' lives in ways that may, through different mechanisms, translate into obedience and endorsement" (pp. 2-3).

So the nature of rebel governance, the degree in which it is institutionalized and the collaboration between rebel and victim around the payment mechanism are all important in determining welfare outcomes.

The welfare outcomes of payment/extortion

Non-compliance with the demands of the gang, rebels, militias, or mafia can lead to financial loss, property loss, injury, or death. Some groups inflict harm on the

property owned by the person who is unwilling to comply (see Lynn 1993 for a review of welfare oucome associated with non-payment in France under Louis XIV; Gragert (1997) for welfare outcomes in Japanese Yakuza protection racket; Williams (2009a) for description of injuries inflicted on unwilling 'payers' in Iraq; and Mason and Campany (1997) for punitive system of *Sendero Luminoso* in Peru).

In return for paying, businessmen, communities, and villages expect protection; however only a few articles even mention the effectiveness of paying and the evidence appears to be entirely context specific. A United States military officer, referring to the effectiveness of the militias, stated that, "People count on the militias…. It's like the mob—they keep people safe" (qtd. in Williams 2009a, 236). According to Reij Al Talata community leaders, who referenced the attack against the Katilingei village, the payment of protection money in Darfur is crucial to a village's survival (Jaspars and O'Callaghan 2008, 14).

Even if protection is secured through payment, there are still consequences on the livelihoods to the individual or community paying protection. Although all victims experience a loss in money or goods, the impact on livelihoods is not widely discussed. Jaspars, et al. (2007, 15) point to the short-term effects, such as loss of income, that paying protection money may have on individuals and communities, but they maintain that what is garnered in long-term benefits, such as retaining access to land or mobility, may be beneficial. In the case of migrant Chinese businessmen in New York City, the authors contend that money taken for protection was only mildly parasitic, while the cost for not paying was far greater (Kelly, et al. 1993, 263).

Moreover, paying a group money to protect against an attack, does not necessarily ensure that other forms of violence will not be committed by the group against community members. Williams (2009a, 157) summarizes this predicament well in stating "protection and predation are two sides of the same coin". In his discussion of protection in the Democratic Republic of Congo, Raeymaekers (forthcoming, 10) notes that rebels proceeded in a reign of terror on the same villages they were being paid to protect. During the 2002 massacre in Kariobangi, people of the local community stated that although the Kenyan Taliban was charged with protecting them, they were "terrorising residents instead of protecting them" (qtd. in Anderson 2002, 532). In other words, payment does not guarantee protection, however in many documented cases it does increase the likelihood that you will be spared from negative outcomes.

From the available literature, reviewed above, we see that there is much description on the nature of protection as set up through mafia and rebel governance structures, with references to predictability and regularity of payments and; some description on the linkages between extortion and protection. Absent is any analysis, in particular empirical work or evidence of how extortion, or the nature of rebel extortion, relates to outcomes for the victims. In the following analysis we attempt to provide an empirical analysis of how institutionalised systems of extortion are able to insure against negative outcomes and even provide the basis for positive welfare returns, as compared to non-payment or *ad hoc* extortion. We develop some testable hypotheses for protection/extortion and welfare. We first discuss the case of Burundi and identify the relevance of the literature to this case.

Conflict and Extortion in Burundi

A short political history of the conflict

Burundi has, until recently, been involved in long and a brutal civil war that left hundreds of thousands of people dead, maimed or displaced. Massive bloodshed took place in 1972, where the armed forces slaughtered between 80,000 and 200,000 (mostly wealthy and intellectual) Hutu. In 1988, responding to a similar uprising in the Ngozi and Kirundo provinces where several thousand Tutsi were killed, 'peace' was restored by the government forces killing 20,000 Hutu (HRW, 1998). Although no formal investigation into the 1988 massacres was allowed, president Buyoya sought for means to reconcile Hutu and Tutsi rather than controlling the Hutu majority by repression. Several Hutu were appointed to government positions.

Many Tutsi, however, viewed loss of ruling power and the resulting massacres in neighbouring Rwanda as a warning sign for what would happen if they started sharing power with the Hutu. They therefore resisted Buyoya's reforms with unsuccessful coups in 1989 and 1992. Despite internal opposition, elections were held in 1993 and on July 1, Melchior Ndadaye became Burundi's first Hutu president. The newly installed president made important changes in the local administration and planned to reform the almost exclusively Tutsi army to increase ethnic and regional diversity (HRW, 1998).

The attempts to reform, however, ended brutally with the killing of Ndadaye in October 1993. The eruption of Hutu-led violence following the killing of the president was retaliated by massive indiscriminate violence by the government forces. HRW reports: "The army responded with clashes on Hutu making no distinction

between communities involved in violence against Tutsi and those that were not. In a period of only a few weeks anywhere from 30,000 to 50,000 people were slain, roughly an equal number from each ethnic group." Uvin (1999) elaborates: "On October 21, 1993, low-level soldiers killed President Ndadaye and other dignitaries after only three months in office, with at least passive support from the highest levels of the army."

In subsequent days and weeks thousands of Tutsi were brutally killed, especially in the north and centre in a campaign led by local Hutu politicians. The army moved in to restore order, killing thousands of Hutu in the process. In total, it is estimated that 50,000 to 100,000 persons were murdered in the three months after the coup; one million fled the country; and hundreds of thousands were internally displaced. 'The scope and intensity of the violence was unprecedented and resulted in a gruesome civil war that lasted sixteen years, leaving the country and its citizens in ruins.'

Rebel governance: extortion without service provision

The civil war in Burundi was a low-tech war in which physical strength, rifles and machetes were the most important assets of the warring parties. Civilians paid heavily for the war: many people were killed, kidnapped or displaced. Women were raped, houses destroyed, cattle stolen, trees burned. Importantly for the topic of this paper, civilians were forced to contribute to the war effort, either by paying contributions, *'cotisations',* in cash or by having support extracted in kind.

The cycle of violence followed a typical pattern: a warring faction would attack an army post or a symbol of government power in a specific locality after which the

army would use disproportionate force to retaliate against the population of that locality. The army considered the population as supporters of the rebels wherever an attack was launched and the rebels considered the population as the base from which to operate. Army and rebels shunned open large scale battles and preferred to rob and punish the civilian population instead.

Burundi's hilly terrain and dispersed population makes it almost impossible for the army to control the entire territory. It was relatively easy for the rebels to hide in, or retreat to, the forest or to move at night through the fields and swamps. Warring factions had their local support bases among the population, most fighters were the sons of Hutu peasants who joined the ranks of a rebel faction whereas young Tutsi men would join the army. This does not mean however that all peasant families were ready to give even more support in terms of labour and cash. They had already 'given' their sons and in some cases their daughters to the rebel group and were too poor to contribute more. The remaining male and female labour was needed on the farm as well as in the household together with the few livestock owned and the meagre cash resources.

Since rebels have to eat, need money to buy weapons and need labour to carry food and weapons they would 'extract' labour and demand cash contributions from civilians who did not volunteer this kind of support or did not want to give any kind of support at all. Our paper focuses on this kind of forced contributions, in cash as well as in kind. Different types of contributions were required. One typical pattern took the form of small groups of rebels that would stop buses passing through their territory, ask all passengers to get out of the vehicle and steal their valuables such as

watches, earrings, rings, shoes and money. From the point of view of the victim, this type of 'extortion', while always a possibility, was irregular and unpredictable in terms of timing and the actual resources extorted. Male able-bodied men and women were not only robbed of their belongings, depending on the needs of the day they were forced to follow the rebel group and carry food, clothes, luggage and ammunition. These men could be held in captivity to perform labour tasks as long as the rebels deemed necessary. In extreme cases, when tensions ran high, for example because of recent political events, bus passengers could be executed on the spot.[i]

Another form of extortion was in the form of 'home' visits where the resident was asked to contribute money. This was a regular occurrence. One of the authors held in-depth discussions with well-informed persons upon several research visits to Burundi between 2011-2013 on rebel taxation and rebel governance during the civil war. It became clear from these discussions that the rebel movements systematically collected contributions from the population in the area under their control. Every month, each adult in the area was visited by a representative of the rebel movement to collect a tax. The level of the tax depended on the occupation and the perceived income of the resident. Persons with a salaried job had to pay more than ordinary farmers. Upon paying the contribution/tax one received a receipt that proved that you paid your contribution. Next to the monetary payment, rebels would also ask for food, for example a small animal like a goat or even a cow. They did not take all of your assets or belongings. If you had four goats for example, they would request one. And a few months later they would come back to ask another one. Asked by the researcher if the population received something in return for these contributions, the answer was always negative: the rebel movement did not provide any services. When they would

intervene in conflicts between neighbours, for example on land issues, it could hardly be called justice, it was more about settling scores and denouncing other people. The researcher then asked if the 'service' the rebels offered may have consisted of the absence of pillaging, in the sense that once you paid your contribution, you were left alone. This was confirmed by the all interviewees. One interviewee told the story of a large cement company in the capital whose owner approached the rebel leader to make a payment per truck of cement in a large road building project. After agreeing on the payment the two sat together and shared a glass of beer, a sign of collaboration and friendly relations in Burundi.

The receipt which counted as proof of one's contribution was particularly useful in the case of traders or entrepreneurs who have to transport goods across municipalities or provinces. When stopped at roadblocks by rebels, the trader was asked to show this receipt upon which he or she could carry on with their business. This was also the case for a passenger who was able to show such receipt when his/her bus was ambushed: (s)he was left alone. The practice of writing receipts for contributions to the coffers of a rebel faction was also implemented on trucks transporting goods. A driver passing through a rebel held territory had to show his receipt before being allowed to pass through. When he did not have it, he was asked to pay on the spot, receive a receipt and was then allowed to continue the journey.

This practice of collection of contributions shows the extent to which rebel factions implemented, and institutionalised, a tax administration in their territory. One could then ask, what kind of protection (or services) were provided by a rebel faction in return for such tax collection? And did these payments really provide some insurance

to civilians against future negative outcomes? Civilians may indeed be ready to contribute if they know that social or other services are provided by the collector. This is particularly the case for collective goods such as security, schools, hospitals or roads. Rarely, however, did the rebel factions have complete control over an extended area for a prolonged time-period, say several years.[ii] As we described above, these armed groups were mobile and moved the war theatre from province to province. As a result, they did not invest much in taking over the role of the state in the provision of collective goods such as health centres and schools. One obvious exception, however, was the collection of taxes. In the areas where armed rebels were operating the government of Burundi was unable to provide security to the civilians as the rebel faction had broken the monopoly of violence. The ability to extort or tax civilians was therefore in the hands of the rebel faction. As described in the work of Arjona (reviewed earlier), while services were not provided, the rebels relied on the civilians for medium term provision of funds, food and supplies. In return, the civilians were not robbed from all of their belongings and were not attacked until the next round of tax collection.

Before we develop our model, it is worth repeating our main research question: do contributions of civilians to the rebel faction provide protection/insurance against negative outcomes? In other words, when a civilian contributes in cash or in kind, does that mean that he and his family will not fall victim to attacks, assaults, property destruction, rape, torture or theft? We can broaden this question to welfare effects in general: do civilian payments protect civilians from experiencing negative shocks on household welfare? Moreover, does the institutional arrangement of the extortion affect the outcome?

We end our description of the (absence of) rebel governance and taxation in Burundi with the formulation of several hypotheses that will be directly tested with the data we were able to collect.

Hypothesis 1: Extortion , on average, insures against negative downturns in consumption welfare

Hypothesis 2: Extortion enables an improvement of welfare over time due to the protection/insurance function.

Hypothesis 3: Regular, institutionalised payments lead to different outcomes that irregular, *ad hoc* payments.

Hypothesis 4: Rebel groups target citizens for extortion depending on a range of socio-economic factors.

Given the Burundi war context of high levels of violence, civil war and extortion we would expect people to pay the extortion money (a tax) and for these people to have better outcomes. In the Appendix we develop a simple model of extortion from the viewpoint of a typical citizen confronted with a rebel faction. The model is based on the perception of the citizen with regard to the efficiency of the rebel movement. If the citizen believes his non-payment is unlikely to be detected, he will not pay the tax.[iii] We face a selection problem if many people who didn't pay the bribe were

killed in response to non-payment. The attrition analysis in the appendix show that this is not a significant problem for our data analysis.

The Burundi Household Priority Survey 1998-2007

The data we use in this paper consist of a panel with two data points over nine years, 1998 and 2007.[iv] In 1998, the World Bank and the Burundi Institute of Statistics and Economic Studies (BISES) conducted a nationally representative general-purpose household survey to analyse living standards. For this survey, 3908 rural households were interviewed (Republic of Burundi, 1998). We designed the 2007 Priority Survey (henceforth PS07) as a follow-up to the 1998 Priority Survey (henceforth PS98). Due to budget limitations, it was impossible to try to track and re-survey all 3908 rural households (391 survey sites) included in the PS98. Therefore, we decided to randomly draw 100 of the 391 baseline sites with the purpose to track and re-survey all 1000 original (1998) rural households in these sites.[v] We trained 65 interviewers during a one-week training during which we improved the questionnaire. The questionnaire was pilot tested in an out of sample village and final corrections were made. The interviewers were instructed to track and re-interview, within each hill, the 10 original households. Overall we managed to locate and re-interview 874 of the 1000 selected households. The supervisor of each team of interviewers undertook a community-level survey in which (s)he asked questions on infrastructure, history, population, attacks and war-related violence.

In a module in the questionnaire on violence-related shocks and their consequences for households and individuals we asked our interviewees if they had to perform forced labour for the rebel movement and if they were asked to pay contributions to

the rebel movement. The latter are known in Burundi as '*cotisation*' and can either be in cash, in kind or by providing physical labour. If an interviewee told as that (s)he had to perform physical labour we also asked him/her the number of times this occurred between the first round of the survey (1998) and the second round (2007). If the interviewee contributed in cash or in kind we asked for the total value (in Burundese Franc) of his/her contributions over the 9 year period.[vi] One limitation of the data set is that we only have the number of times the person was extorted (as well as the amount in the case of cash) but we do not have the exact timing of each act of extortion.

Descriptive results

In this section we provide some data to illustrate the patterns of payment. As shown in table 1 below, 70 per cent of the sample did not incur any type of extortion over the 9 year period (1998-2007). From the 30 percent who report having been extorted by paying contributions in cash or through forced labour, 23 per cent of the sample had made regular payment of cash to the rebel groups and 14 per cent had to provide labour. 6.6 per cent had provided labour alone and around 16 per cent had provided contributions in cash only. A substantially higher proportion of females reported no extortion (81 per cent as opposed to 67 per cent of males).

< TABLE 1 ABOUT HERE >

For purposes of exposition and analysis we are interested in understanding the insurance function of extortion, and in particular cash payments as opposed to non-payment and forced labour extraction. We use cash payments to proxy for regular and

institutionalised 'insurance' extortion and forced labour to proxy for irregular and unpredictable extortion. First we analyse whether extortion of any kind has a significant and positive effect on welfare, as compared to non-payment. We then disaggregate this analysis by the nature of payment. Table 2 below provides statistics that allow us to compare the different characteristics of the groups that are extorted in different ways.

< TABLE 2 ABOUT HERE>

The data provided in table 2 above give insight into the profiles of households that paid some kind of extortion. In terms of the change in income/expenditures over the ten year period (income_07- income_98), the unconditional mean shows that the change has been positive for all categories except the labour only payment (col 7). However, the change has been significantly larger for those being extorted (col 2), versus those not being extorted (col 1). This may point to evidence of an insurance function related to extortion. Disaggregated by extortion type we see that the positive change is dominated by those households who have paid cash, not labour. In fact for those household who have had labour forcibly extracted we see a negative change in welfare (col 7). This also suggests that our hypotheses on regular and 'institutionalised' payments (hypothesis #3) are more likely to perform an insurance function than random and unknown extraction of labour.

The data also suggest, as expected, that observable characteristics are likely to predict extortion type. For instance, elderly people are less likely to be extorted on both counts (cash and labour). Female headed households are less likely to have extortion

(81 per cent versus 67 per cent of male headed households). Education is not obviously related to extortion. Higher levels of land, assets and enterprise ownership are also correlated with extortion, providing support for the link between socio-economic profile and extortion (Hypothesis #4).

Welfare results

In this section we use econometric methods to test whether extortion has 'welfare'/protection effects as opposed to no extortion. We also test whether the type of extortion matters for welfare outcomes (measured in terms of current consumption and changes in consumption over the 9 year period). We first use a simple OLS linear specification

$$LnC_{ij07} = \alpha_0 + \alpha_1 LnC_{ij98} + \alpha_2 E_{ij98\_07} + \alpha_3 Z_{ij98} + \delta_j + \varepsilon_{ij} \qquad (1)$$

where C is the consumption in year 2007 or 1998 measured in adult equivalents for person *i* residing on province *j*, E is a dummy variable capturing whether or not the individual was extorted in the period between the two survey rounds (with different dummy variables for each type of extortion), α_2 our coefficient of interest, Z a vector of individual, household and community level characteristics (age, sex and education of the head of the household; three types of assets as well as the number of death and wounded from violence), δ the province fixed effect and ε a random, idiosyncratic error term.

The results are presented on table 3 below. We use changes in consumption per adult equivalent between 1998 and 2007 as our dependent variable. We include

consumption at baseline, our extortion dummy variable and a set of characteristics as in equation (1). Province level fixed effects are included in the specification. We performed the same regression (results not shown) with consumption in 2007 as dependent variable and reached the same results (apart from the coefficient on consumption at baseline).

<TABLE 3: ABOUT HERE>

Three results from this regression, while not surprising by themselves, nevertheless buttress the confidence we have in the quality of the data exactly because they confirm what other researchers have found: (i) the coefficient of consumption at baseline is negative and statistically significant at the 1% level, echoing the well-know convergence result in other panel data studies; (ii) the effect of violence on a consumption growth is negative and statistically significant at the 5% level and (iii) the effect of education is positive and statistically significant at the 1 or 5% level depending on specification. These established results may give some confidence to the other results obtained in table, most notable the coefficient of interest, α_2 , on the variable *extortion*, which is positive and statistically significant in three of the regressions, always involving cash payments. Also notice the positive effects of age at baseline and of having a female head of the household at baseline on changes in consumption over the conflict period. Higher initial asset holdings also predicts positive and significant changes in welfare.

The welfare results derived from an OLS estimation only make sense in absence of selection bias. It is, however, unlikely that the latter is absent. More specifically, the

rebel groups may know whom they have to extort/tax because there may be some observable indicators of wealth, such as doing business of asset holdings. This means that it is necessary to deal with potential selection effects in our welfare estimation. We do that by first performing a probit analysis to explain the determinants of forced contributions, for cash as well as for labour and subsequently employ matching methods to estimate the effect on welfare.

The matching approach originated from the statistical literature and shows a close link to the experimental context. Its basic idea is to find in a large enough group of non-participants who are similar to the participants in all relevant pre-treatment characteristics X. That being done, differences in outcomes between this control group and those of the participants can be attributed to the 'treatment', in our case the extortion (we refer to Caliendo and Kopeinig, 2005 for full treatment of this method). Since conditioning on all relevant covariates is limited in the case of a high dimensional vector X, Rosenbaum and Rubin (1983) suggest the use of balancing scores $b(X)$, i.e. functions of the relevant observed covariates X such that the conditional distribution of X given $b(X)$ is independent of assignment into treatment. One possible balancing score is the propensity score, i.e. the probability of participating in a programme given observed characteristics X, which we will use.

In our particular case, propensity score matching (PSM) involves estimating a binary treatment model (in out case a probit model) that predicts the probability of each household being targeted for one of five types of extortion as a function of observed characteristics. The variables included in the analysis are those that influence simultaneously whether a household in involved in extortion and the outcome of

interest which is increased income. We use Stata command *pscore* to identify the matching in the results below.

The results of a probit extortion model are shown in table 4 (province fixed effects were used but for presentational purposes are not reported here). Younger people are significantly more likely to be extorted under any form older people. Males are significantly more likely to be extorted under all categories except for *cash only* than females. Education does not predict probability of extortion. In this way, observable characteristics of individuals and households predict extortion. This makes sense, as it is often a series of observable characteristics that rebels use as a means of targeting people for purposes of extortion. Looking at the *enterprise* variable we see that those households owning an enterprise in 1998, they were significantly likely to be targeted for cash extortion than those with no business enterprise. Enterprise ownership does not predict labour extortion (again supporting hypothesis #4).

<TABLE 4 ABOUT HERE >

The negative effect of consumption on the probability of being extorted is surprising, even after controlling for enterprise ownership and other characteristics. We expected a positive sign here. It may be an indication that the rebels operate in a similar way as the Chicago mafia in taxing the production of pasta, to wit taxation not based on the consumption of the factory owner but on his production, production capacity or his assets. It could also mean that poor people are targeted because they are easier to extort, with low levels of consumption signalling powerlessness.

The effect of violence varies depending on the type of extortion: households residing in areas of high violence are significantly more likely to have cash extortion than households in less violent areas. Labour extortion is not related to the level of violence in the area, implying that households in any area is equally likely to have labour extorted.. We are not able to distinguish the chronology of taxation and violence as we do not know which came first. Note however that most of the violence in our dataset occurred in the first few years (1999, 2000, 2001) of the period under study, making it unlikely that taxation preceded or indeed explains the occurrence of violence. Violence and taxation are consistently and positively correlated across the five regressions.

Moving to the results of the matching methods we first need to test the probit model specification for equality of the mean and standard deviation of the observed characteristics across extorted and non-extorted households. The test is called the balancing propensity tests (Rosenbaum and Rubin 1983; Heckman et al. 1997; Dehejia and Wahba, 2002). The next step in the PSM involves testing the 'match'. This means using the propensity scores estimated in the first instance to identify the non-extorted that compares to the extorted (ie, with the closest propensity score values) using the 'nearest neighbour' algorithm. If a matched sample can be obtained then it is possible to estimate the impact of extortion/taxation on the household's income (welfare).

Our results indicate that the balancing property is satisfied for all five model specifications for the extorted versus non-extorted. The number of households in each of the 6 blocks of the propensity score is shown in the Appendix. So, for instance, for

the first model – any extortion versus non extortion—we see that for the lower bound of the propensity score (0 probability of being selected into extortion) we have households that are comparable and whose characteristics satisfy the balancing property – 207 non-extorted directly comparable to 21 extorted -- and so on. This comparability is confirmed by looking at the distribution of the propensity score according to extorted and non-extorted households (see figures in annex). Again, for the first model, for households who are extorted we can see that the propensity score distribution is more of a normal curve than for the non-extorted, however there is substantial overlap implying that there is an adequate common support to enable us to predict the effect of extortion on welfare using matching methods.

< TABLE 5 ABOUT HERE >

Using matching methods, Table 5 presents the effect of extortion in the 1998-2007 period on welfare in 2007, taking account of the selection effect considered above. As in the case of the OLS estimation we find that extortion has a positive and statistically significant effect on welfare, suggesting an insurance or even welfare improving role of cash payments (supporting hypotheses #1 and #2). Having cash extorted by the rebel group is associated with an increase of between 16 and 25% in adult equivalent consumption depending on specification. On the other hand, extortion under the form of forced labour contributions does not have any effect on future welfare.

Robustness Analysis

Unobservables

One may be concerned that selection into extortion is driven by unobservables. In that case the extortion variable in the welfare analysis partly captures such effect. In order to address this concern we apply a method proposed by E.Oyster (2013, NBER and University of Chicago). She writes that we can learn something about the effect of unobservables on the coefficients of interest when looking at the inclusion of additional observables combined with the movement of the R^2. This is the case under the assumption that selection on observables is proportional to selection on unobservables.

Oyster (2013) wrote a STATA command 'psacalc' (proportional selection assumption) to calculate delta (δ), the degree of proportionality between the observed and the unobserved variables. If delta=1 this means that the observed and the unobserved have an equally important effect on the coefficient of interest. We compare the coefficient of our extortion variable in a model with and without observables and calculate the delta. With one more more assumption to make, on the maximum value of R, we can compute B*, which is the value of our coefficient of interest corrected for bias attributable to the unobervables.

In a regression without observables, B=0.13 and R^2=0.003

In a regression with observables, B=0.16 and R^2=0.32

The psacalc command allows us to calculate delta in this instance, and we find δ=0.4. This already indicates hat the unobservables have less effect on our coefficient of interest than the observables. From this we can calculate B* using the formula in Oyster(2013, p.9)

$$\beta^* = \tilde{\beta} - \delta \frac{\left(\beta' - \tilde{\beta}\right)\left(R_{\max} - \tilde{R}\right)}{\left(\tilde{R} - R'\right)}$$

Whereby B' and R' stand for the coefficient and the R^2 from the regression without observables and β~ and R~ for those of the regression with observables. Implementing this formula for our data yields a B*=0.175 with Rmax=0.7 and B*=0.185 with R_{max}=1. These are both very reasonable coefficients whose magnitude is not far from our base result (0.16) and which is a bit lower (but certainly in the reasonable ranch) of our matching result. Even with delta=1 and R_{max}=1, B* would be 0.22, still in the reasonable ranch.

Growth at the province level

One could be concerned that province level growth trajectories may be responsible for the observed effect, rather than the extortion mechanism. We test this possibility by including province level growth rather than province fixed effects in our analysis. We first remark that over our entire sample, growth of household level consumption is negative, from 8.63 (in logarithms) to 8.52. This is a first indication that there has not been post-conflict growth in Burundi at the time of the survey (2007). In effect, growth is negative in 6 out of the 12 provinces in our sample.

When we include province level growth as a regressor in stead of province fixed effects, the coefficient of the extortion variable increases from 0.16 to 0.18 (for any extortion) and from 0.25 to 0.26 (any cash). The coefficient of household consumption at baseline increases from -0.75 to -0.63. This means that province level growth trajectories cannot account for the effect we observe on extortion. Since they are much more specific in the effect they capture compared to the province fixed effect, these growth trajectories reduce the effect of household consumption at baseline somewhat, which is entirely plausible.

An additional concern may be that the extortion variable is picking up effects of economic recovery if regions with higher recovery concide with regions with more extortion. Below we present a graph that plots the province level growth 2007-1998 against the probability to be extorted (any extortion). It can be seen visually that there is no relationship. We also computed the Pearson correlation coefficient, which is 0.01 and which is not statistically significant using the usual thresholds. For completeness we have performed the same analysis at the village level and we found a correlation of -0.09 whih was not statistically significant either at the usual thresholds. Graphs 1 and 2 show the scatterplot and one can visually observe the absence of a relationship.

<Graph 1 and Graph 2 about here>

Pre-1998 violence and convergence

Households affected by pre-1998 violence (captured by the number of death and wounded in their village from 1993 to 1998) have lower consumption at baseline (1998) than non-affected households. See table 6. This could indicate that this lower consumption is the result of pre-survey violence, after which a recovery to pre-war levels could follow (convergence) that is not related with extortion. We notice that consumption at endline (2007) is also lower in the areas affected by pre-1998 violence. The reduction in consumption in 2007 in the pre-1998 affected areas is less than in the non-affected areas, hence the effect is less negative, but the difference between the two (Difference in Differences) is not statistically significant at the usual

thresholds. Hence this cannot account for the positive effects we observe from the extortion variable.

<Table 6 about here>

In order to verify if the result can be explained by post-war growth or convergence, we show an additional table with four groups of households: (1) those not affected by extortion (household level), nor by civil war (village level death and wounded 1993-1998); (2) affected by extortion (any type), but not by civil war violence; (3) not extorted but affected by village level violence and (4) affected by both. Table 2 below shows the welfare change in the four groups. Only group (2), the extorted group, has seen its welfare increase, while the three other groups saw their welfare decrease.

<Table 7 about here>

Stability

One may be concerned that is it stability rahter than extortion in rebel held areas that explains our result. If we proxy stability by the absence of deaths and wounded from civil war (which is the violence variable in our paper) than we already control for the effect of stability in our analysis. Stability indeed has a positive effect on consumption growth, but it does not capture or diminish the effect of extortion.

Conclusions

This is an empirical paper that allows us to test for the linkage between taxation in the form of extortion payments in a war context and household welfare outcomes, as well

as the nature of the payments in relation to outcomes. We show that a person's socio-economic profile determines his/her likelihood to fall victim to one or another type of rebel taxation. We also find that payments in the form of cash (a regular, institutionalised form of payment which we use as a proxy for 'stationary rebels') increase household welfare by between 16 and 25 %. Extortion in the form of labour (a proxy for ad hoc payments and 'roving rebels') does not have a welfare enhancing effect. These findings tell a story about rebel governance in times of conflict, suggesting that where rebels have some legitimacy and rebel taxation is institutionalised within the governance structures, civilian populations may be provided with extra-legal security that can ultimately enhance their welfare. Whether their welfare would have improved in the absence of conflict is not something that we can test here. Conflict is not an RCT ! Group 1 in table 7 however shows that conditions in Burundi deteriorated over time, even for those not affected by conflict or extortionIt is clear that relationships between extra-legal actors and civilians in times of war may actually be mutually beneficial: a results that supports Arjona's conclusions. Our results suggest that the extent of institutionalisation of the extortion appears to be critical in obtaining a positive result. Regular and predictable extortion are more likely to insure positive outcomes than unpredictable extortion, which, on average, has a welfare reducing outcome.

The work by Sanin and Baron, and Olson (reviewed in the introduction) resonates with what we see in the Burundi case above, where rebels use cash-and-receipt taxation to securitize welfare of the citizens, however, *ad hoc* labour extraction remains punitive and coercive, with no welfare or security outcomes. The findings presented here also speak to Arjona's (2008) theory of local orders within the context

of civil war. She highlights the multiple strategies that armed groups can opt in establishing social order during and after civil war. These are *coercion* (corresponding the exclusive use of violence and lack of rule), *minimal* (regulates violence and secure basic resources but stays out of civilian affairs*), indirect* (rules civilian affairs by proxy) and *comprehensive (*overtly regulates civilian affairs, such as public goods, religion). The cash-and-receipt security payment system used in Burundi corresponds largely to the *minimal* rebel strategy to promote social order, whereas the labour extraction corresponds to that of *coercion*. Within the Burundi civil war context , due to the fragmented and geographically dislocated nature of war, disparate rebel movements found it difficult to use indirect and comprehensive strategies to establish order. Our works illustrates the co-dependence of the civilian population and the rebel movement, but fundamentally it shows how institutionalized forms of criminality have better outcomes for victims of extortion in terms of security and welfare than simple punitive, *ad hoc,* strategies.

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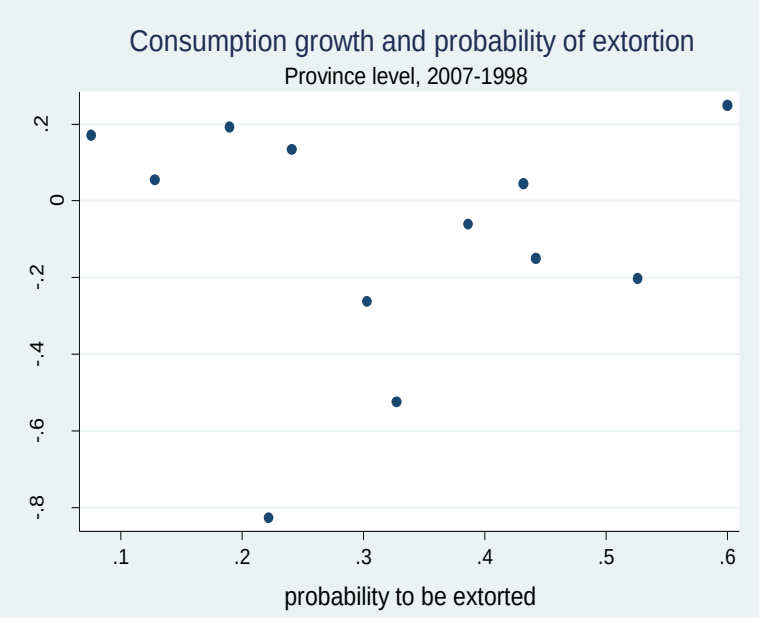
Graph 1



Graph 2

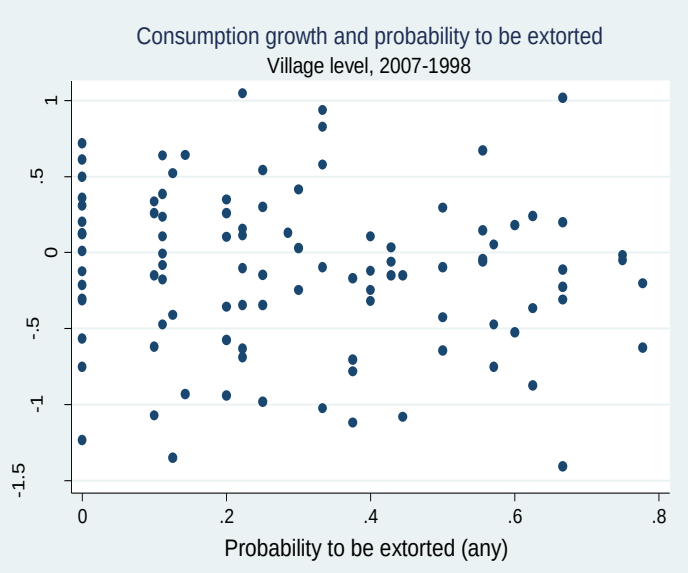


Table 1: Type of extortion by gender of household head (sample size and mean)

	Nature of extortion	**Male**	**Female**	**Total**
1	No extortion	450	157	607
		66.57	81.35	70.07
2	Any Extortion (cash or labour)	226	36	260
		33.43	18.65	29.93
	Total	676	193	869
3	*Any Cash*[vii]	*173*	*29*	*202*
		27.77	*15.29*	*24.97*
4	*Any Labour*	*114*	*11*	*125*
		20.21	*6.55*	*17.08*
5	*Only Cash*	*112*	*25*	*137*
		19.93	*13.74*	*18.41*
6	*Only Labour*	*53*	*7*	*60*
		10.46	*4.27*	*9.0*

Table 2: Characteristics of different groups

	1 (no extort)	2 (any extort)	4 (any Cash)	5 (any Lab)	6 (only_ cash)	7 (only_ lab)
Income 98	7284	6618	6538	6241	6962	6887
Income 07	8019	8066	8714	7547	8539	5883
HH_size_98	5.05	5.22	5.21	5.36	5.09	5.12
HH_size_07	5.45	5.93	5.83	6.29	5.60	6.19
Age 98	44.47	39.24	40.07	37.7	40.61	36.16
Female head	81.3	18.65	15.03	6.05	12.95	3.03
Male head	66.57	33.43	25.59	20.1	16.57	7.84
Educ_98	0.34	0.34	0.34	0.37	0.32	0.37
Enterprise98	0.14	0.20	0.23	0.18	0.23	0.16
Livestock98	0.66	0.58	0.65	0.55	0.70	0.67
Asset_98	0.57	0.70	0.65	0.69	0.71	0.45
Land _98	0.49	0.54	0.53	0.59	0.51	0.61
Land _07	0.46	0.59	0.51	0.60	0.47	0.63
Deaths/w_98	67.12	66.91	60.81	68.12	57.96	56.96
Deaths/w 07	1.94	6.17	4.73	6.19	4.06	5.67
Torture	0.07	0.48	0.32	0.51	0.24	0.30
Prison	0.09	0.15	0.15	0.19	0.12	0.16
handicapped	0.08	0.09	0.08	0.10	0.07	0.09

Table 3: OLS estimation results for the effects of different extortion types on changes in welfare 2007-1998

Extortion type	1 (any)	2 (any cash)	3 (any lab)	4 (only_cash)	5 (only_lab)
ln98	-0.75***	-0.76***	-0.76***	-0.74***	-0.74***
	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)
Extortion	0.16**	0.25***	0.09	0.25***	-0.11
	(0.07)	(0.08)	(0.09)	(0.087)	(0.11)
sex98	0.22**	0.21**	0.27**	0.19*	0.24**
	(0.09)	(0.09)	(0.10)	(0.09)	(0.10)
age98	0.005**	0.005**	0.005**	0.006***	0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Educ Head	0.18***	0.19***	0.16**	0.20***	0.16**
	(0.06)	(0.07)	(0.07)	(0.08)	(0.08)
Enterpr98	-0.04	-0.09	-0.02	-0.16	-0.11
	(0.09)	(0.09)	(0.10)	(0.10)	(0.10)
ln_livestock98	0.014*	0.013*	0.015*	0.014	0.015*
	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)
asset1998	0.14***	0.16***	0.15***	0.14***	0.13***
	(0.03)	(0.028)	(0.032)	(0.03)	(0.03)
violence98_07	-0.005**	-0.005**	-0.006**	-0.007**	-0.009***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Constant	6.30***	6.39***	6.41***	6.20***	6.23***
	(0.52)	(0.54)	(0.55)	(0.57)	(0.59)
Province FE	Yes	Yes	Yes	Yes	Yes
N	867	807	730	742	665
R-squared	0.32	0.32	0.31	0.32	0.31

Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 4: Probit estimation of the determinants of extortion

	(1)	(2)	(3)	(4)	(5)
Variable	**(any)**	**(and cash)**	**(any lab)**	**(only cash)**	**(only lab)**
ln98	-0.058**	-0.055***	-0.040**	-0.018**	-0.007
	(0.023)	(0.021)	(0.018)	(0.009)	(0.013)
sex98	-0.108***	-0.082**	-0.103***	-0.010	-0.038**
	(0.039)	(0.037)	(0.026)	(0.020)	(0.017)
age98	-0.005***	-0.003***	-0.004***	-0.001***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Educchef	-0.013	-0.018	0.009	-0.015	0.008
	(0.034)	(0.031)	(0.027)	(0.016)	(0.018)
enterpr98	0.064	0.080**	0.010	0.045*	-0.013
	(0.041)	(0.041)	(0.031)	(0.025)	(0.020)
ln_live	0.004	0.004	-0.000	0.003*	-0.000
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
asset1998	0.012	0.010	0.006	0.005	0.001
	(0.014)	(0.013)	(0.012)	(0.006)	(0.008)
dw98_07	0.003**	0.004**	0.001	0.003**	0.001
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Obs	867	807	730	742	638

Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 5: ATT results from matching methods

Extortion type	(1) (any)	(2) (any cash)	(3) (any lab)	(4) (only cash)	(5) (only lab)
OLS	0.16**	0.25***	0.09	0.25***	-0.11
	(0.07)	(0.08)	(0.08)	(0.09)	(0.10)
Matching	0.14*	0.23***	0.004	0.25***	-0.14
	(0.08)	(0.08)	(0.09)	(0.11)	(0.13)

Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 6: pre-1998 violence and consumption growth

Pre-1998 violence	Consumption 1998	Consumption 2007	Diff
Yes	8.53	8.44	-0.09
No	8.76	8.62	-0.14
	0.23***	0.18***	0.05

Table 7: Extortion, Violence and consumption growth: 4 groups

Group	Consumption 1998	Consumption 2007	Diff
No extortion, no violence	8.68	8.55	-0.13
Extorted, no violence	8.57	8.60	+0.03
No extortion, violence	8.51	8.24	-0.27
Extortion, violence	8.58	8.44	-0.14

Annex 1: A simple model of extortion and protection

In order to motivate some testable hypotheses we develop a simple framework for the choice facing a potential victim when deciding whether or not to make an extortion payment. To simplify notation we use the term 'rebel tax' to refer to a payment made as extortion or protection. Potential victim, V, can pay a rebel tax, T, or pay no tax, 0. If V pays T, the probability that V will be attacked/harmed, A, by the perpetrator (in this case the rebel faction) is p. If V does not pay T, the probability of attack is p', with $p < p'$. If V is attacked, the associated utility is U_a and when V is not attacked it is U_n , with $U_a < U_n$. The expected utility in both cases is as follows:

$$T \dashrightarrow p(A) = f(T,\theta) \quad and \quad E(U(T)) = p(U_a) + (1-p)(U_n) - T$$

$$0 \dashrightarrow p'(A) = f(0,\theta) \quad and \quad E(U(0)) = p'(U_a) + (1-p')(U_n)$$

With p(A) and p'(A) being the probability of being attacked as a function of the tax paid and of personal characteristics θ. The expected utility function E is a Von Neumann-Morgenstern utility function with the usual characteristics. This will lead to the following decision making criteria.

V will pay the tax to the rebels when:

$$E(U(T)) > E(U(0)). \quad or$$

$$p(U_a) + (1-p)(U_n) - T > p'(U_a) + (1-p')(U_n), \quad or$$

$$(p-p')U_a + (p'-p)U_n > T \quad or$$

$$(U_n - U_a)(p'-p) > T \quad (under\ the\ conditions\ p < p'\ and\ U_a < U_n)$$

Implying that V will pay the tax when the utility loss in the event of an attack is high (U_a is small) and when the probability of being attacked in the case of not paying is high. When the probability of being attacked, given no payment (p') approaches certainty and the utility after an attack is close to zero (V may even be killed) then V will pay the tax as long as it is smaller than U_n.[viii] The hypotheses that fall out from this model are:

> *Hypothesis 1*: the higher the loss of utility from an attack, the higher the probability that I will pay the tax

> *Hypothesis 2*: the higher the probability of being attacked in the event of not paying, the more likely I will pay

Both of these hypotheses have to do with perceptions, the first has mainly to do with the perception a victim has of their own potential loss, probably linked to their asset and income level; the second is mainly linked to the victim's perception of the efficiency of the rebel movement to detect non-contributors. We cannot test these hypotheses directly, as we have no data on these perceptions. With the above model in mind as well as the context of Burundi outlined in the preceding section we can develop some testable hypotheses below. While these testable hypotheses are not mathematically derived from the theoretical model, they demonstrate the same reasoning as the one exhibited in the model.

Annex 2: Attrition analysis

Table A1: Testing for selective attrition among original households (+)

Household characteristics in first round	Sampled in both rounds	Sampled in first round only	Difference in means with t-test	Interviewed in both rounds	Drop out in second round	Difference in means with t-test
Household size	4.90	4.94	0.039	5.09	3.57	-1.52***
Adult equivalents	3.88	3.89	0.19	4.03	2.85	-1.18***
Age of the head	44.09	43.18	-0.9*	43.05	51.12	8.06***
% head educated	0.32	0.31	-0.0	0.34	0.19	-0.15***
% female head	0.25	0.25	0.0	0.22	0.44	0.22***
Total income	42673	40012	-2260	44248	31941	-12307**
Consumption per ae	7115	6689	-425**	7089	7295	206
Number of cattle	0.46	0.38	-0.08*	0.5	0.19	-0.3**
% poor	0.69	0.71	0.03*	0.69	0.70	0.02
village level						
time to market	2.67	2.69	0.02	2.65	2.8	0.16*
Intensity of violence				4.07	3.62	-0.45
N	1000	2908		872	128	

Table A2: Probit models testing for selective attrition, using FGM method (+)

Household characteristics in first round	Sampled in both rounds (1)	Sampled in both rounds (2)	Interviewed in both rounds (3)	Interviewed in both rounds (4)
Household size	-0.01	-0.013	0.129***	0.126***
Age of the head	0.002	0.002*	-0.01***	-0.01***
head educated	0.016	0.007	0.167	0.177
female head	-0.001	-0.008	-0.287**	-0.346**
Log total income	0.036	0.054*	0.036	0.069
Number of cattle	0.018	0.025	0.09	0.08*
Village level				
time to market	-0.012	-0.011	-0.054	-0.054
Intensity of violence			0.001	-0.001
constant	-1.05***	-6.59	0.56	0.68
Province FE	No	Yes	No	Yes
N	3908	3908	1000	1000

(+) Source : Burundi Priority Household Survey 1998 and 2007. The consumption data of two tracked households were found to be unusable. Probit specifications where the selection variable is 1 for selection into the sample or the interview and 0 otherwise. All models are clustered around survey cites to obtain robust standard errors; Fitzgerald, Gottschalk and Moffitt (1998).

Analysis of attrition in the sample

In the 2007 survey, we managed to track and re-interview 87.4% of the original households and 82% of all individuals interviewed in 1998. These are reasonable figures after a period of 9 years in between the survey rounds, a period characterised by civil war. Well-known panels in developing countries such as the Kenya Life Panel Survey 1998-2003/2005 (84%), the Indonesian Family Life Survey 1993,1998, 2000 (88%); and the KwaZulu-Natal Income Dynamics Study 1993-1998 (84%) have similar attrition rates. Baird, Hamory and Miguel (2008) signal the lower re-contact rates for panels who do not track individual members who moved between survey rounds.

For 113 of the 126 of the drop out households we were able to find out the reason why they dropped out, by asking neighbours and village authorities about their whereabouts. The drop outs in the second round can be divided into four groups: (1) all original household members deceased (24.8%). This mostly concerns small households with old members, (2) the household moved to another place, but we were unable to find where (22.1%), (3) the household disappeared all of a sudden and nobody knows its current whereabouts (21.2%) or (4) the household dissolved (31.9%), mostly parents deceased and children went their own way.

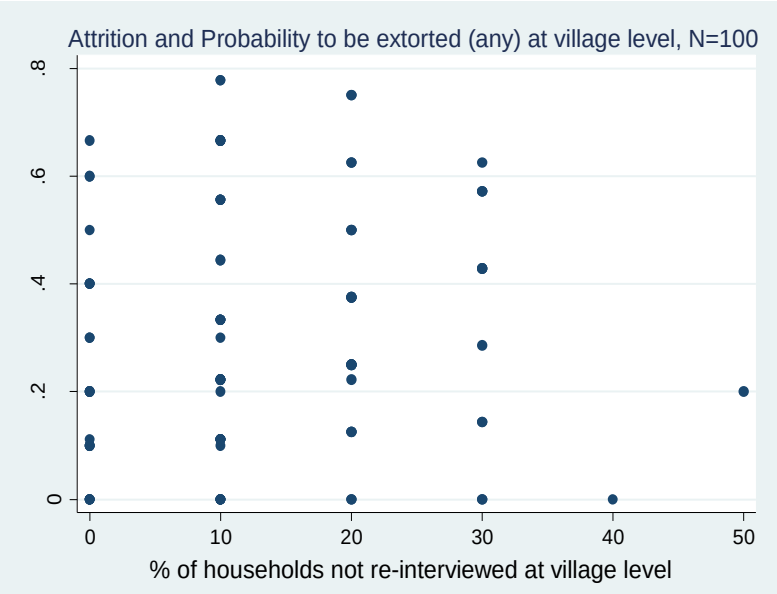
This kind of drop out is not different from drop out in other panel surveys. It is important is to find out whether or not the drop out households differ from the tracked households, which would bias subsequent estimation results. In Appendix 1 we explore potential selection bias in two ways. *First*, we examine mean differences on observables between tracked and drop out households and *second* we perform a probit analysis of attrition similar to Fitzgerald, Gottschalk, and Moffit (1998). We find that drop out households were on average smaller, were more likely to have an older, female or less educated head of the household and had lower total income and less cattle. In terms of poverty, measured as consumption per adult equivalent, the drop outs do not differ from the tracked households. We do not observe a difference for the intensity of violence, measured as the number of deaths and wounded at the village level. The drop out households did have a slightly worse access to markets in 1998.

The fact that the violence in the civil war did not influence, or jeopardize, our tracking of households is an important finding in itself. It means that panel surveys can be conducted safely and with low drop-out rates in a war-affected country, with selection effects not different from those in other panel surveys and with no selection, at least

not in our Burundi sample, on violence in civil war. Furthermore, it implies that the possible real selection bias that could be attributed to the fact that those who do not pay rebel tax are killed and therefore not in our sample, is not a significant problem for our data.

None of our information pointed to the practice of extortion as a reason to leave the area and hence be out of reach of our interviewers. We also included a formal test to demonstrate that extortion is not correlated with attrition in our sample. We compute the Pearson correlation coefficient of the probability to be extorted at the village level (aggregated from household data) and the village level re-interview rate. The coefficient is 0.09 and not statistically significant at the usual thresholds. Graph A allows one to observe that there is no relationship between the two. We have included this in the annex on attrition in the revised version of the paper.

Graph A



Annex 3:

The balancing property is satisfied

This table shows the inferior bound, the number of treated
and the number of controls for each block

Inferior of block of pscore	extort_any 0	1	Total
0	207	21	228
.1666667	236	75	311
.3333333	112	81	193
.5	45	64	109
.6666667	10	20	30
.8333333	0	1	1
Total	610	262	872

The balancing property is satisfied

This table shows the inferior bound, the number of treated
and the number of controls for each block

Inferior of block of pscore	extort_cash_any 0	1	Total
0	259	23	282
.1666667	245	76	321
.3333333	69	56	125
.5	34	43	77
.6666667	3	4	7
Total	610	202	812

The balancing property is satisfied

This table shows the inferior bound, the number of treated
and the number of controls for each block

Inferior of block of pscore	extort_lab_any 0	1	Total
0	445	36	481
.1666667	109	29	138
.3333333	38	34	72
.5	16	25	41
.6666667	2	1	3
Total	610	125	735

The balancing property is satisfied

This table shows the inferior bound, the number of treated
and the number of controls for each block

Inferior of block of pscore	extort_cash 0	1	Total
0	190	4	194
.0833333	182	33	215
.1666667	162	46	208
.3333333	66	44	110
.5	10	10	20
Total	610	137	747

The balancing property is satisfied

This table shows the inferior bound, the number of treated
and the number of controls for each block

Inferior of block of pscore	extort_labour 0	1	Total
0	423	12	435
.0833333	107	15	122
.1666667	62	16	78
.3333333	17	16	33
.5	1	1	2
Total	610	60	670

[i] This type of ambush is very well presented in the short film 'Nawewe' (meaning 'you too') who was nominated for an Oscar in 2010.

[ii] The exceptions were the provinces of Rural Bujumbura and Bubanza were the rebel faction FNL (Front National de la Libération) was the dominant force from the beginning till the end of the civil war. At the time of data collection however, these two provinces were not secure for field work.

[iii] The model has been placed in the Appendix as we are not able to test its implications directly.

[iv] The issue that the first round of data collection did not take place before the start of the civil war will be discussed in the robustness analysis.

[v] To choose which hills to revisit and to end up with the same ratio of selected to unselected sites in each province as in the 1998 survey, we listed, per province, all hills surveyed in 1998 and picked each fourth hill until we selected 100 hills. In the 1998 survey, 10 households were chosen randomly in each survey site. The survey site was the hill (*sous-colline*), the smallest administrative unit.

[vi] The 2007 survey was financed through the MICROCON project (EU-6th Framework) with contributions from the US Institute of Peace and Wageningen University. The research proposal was approved by an Ethical Review Board. During as well as after the survey, to the best of our knowledge, no respondent experienced negative repercussions from his/her participation.

[vii] The categories 3-4 are not mutually exclusive categories as 'Any cash' extortion does not exclude labour extortions that have been made on the same household. Whereas categories 5 and 6 are exclusive categories.